or is liberated by the knife, the escape for a long period is aided by the application of moist heat.

The continued application of moist heat for a long time after the escape of purulent fluid is again, I conceive, indifferent practice. It sustains discharge; it sets up unhealthy decomposition of fluids; it produces a thickened soddened condition of skin, most favourable to the production of sinus; and it retards recovery. When a surface is freely open, and suppurating, dry and not moist heat is the remedy. We are in want in these cases of a simple invention; we require something which we can apply as readily as a poultice which shall keep up the temperature of the part, and at the same time take up moisture, and gently desiccate, without injuring the tissues.—*Brit. Med. Journ.*, May 12, 1866.

MEDICAL PATHOLOGY AND THERAPEUTICS, AND PRACTICAL MEDICINE.

16. *Microscopical Researches on Cholera.*—The most important recent contribution to the pathology of cholera is the series of observations upon the morbid changes which have occurred in cholera made with the aid of the highest powers and new methods of investigation by Dr. L. S. BEALE. This eminent physiologist and skilful microscopist has entered upon this inquiry, he states, as a scientific observer who has neither formed nor adopted any views concerning the nature or treatment of the disease.

"In this inquiry," he says, "it seems to me desirable to start from the alimentary canal. Every one who has seen cholera has been struck by the remarkable characters of the matter discharged from the intestinal tube, and those who have made post-mortems are familiar with the fact that the small intestines almost always contain a considerable quantity of pale, almost colourless gruel, rice, or cream-like matter. This has been proved to consist almost entirely of columnar epithelium, and in many cases large flakes can be found, consisting of several uninjured epithelial sheaths of the villi. I have often found such sheaths in the stools in previous epidemics, and probably every one who has carefully observed the disease will have inclined towards the opinion that in bad cases it is probable that almost every villus, from the pylorus to the ileo-cæcal valve, has been stripped of its epithelial coating during life.

The alteration in the apparatus concerned in the absorption of all nutrient matters from the intestinal canal, and the changes accompanying it and preceding it, are probably sufficient to account for death by collapse. Most important, therefore, it is to ascertain, if possible, the several phenomena of which this denudation of epithelium is the climax, and the order in which they occur. These important organs, the villi, are, in a very bad case, all, or nearly all, left bare, and a very essential part of what constitutes the absorbing apparatus is completely destroyed. If only a considerable portion of a villus was denuded, reparation might doubtless occur by new growth from the cells which remained, but if the villus was entirely stripped, it is more probable that it would waste, and its place be at length occupied by a new one, which would grow from its base, than that epithelium would grow anew from its bare surface. It is probable that the extent of this process of denudation determines the severity or mildness of the attack. If the great majority of the villi have suffered, it is scarcely reasonable to consider recovery more probable than it would be after a very extensive burn or scald. We shall have to inquire what is the proximate cause of the denuding process? Why does the epithelium drop off? What circumstances cause it to become detached? The process may be due to violent contraction of the muscular fibres of the villi and the retraction of the villus within it sheath; but although, no doubt, contraction occurs, it is scarcely probable that the villi would be so generally and completely stripped as they are in severe cases. It seems more probable that the epithelium may become detached in consequence of the almost complete cessation of the circulation in the capillaries beneath, but the death of the cells may occur in consequence of their being exposed to the influence of certain matters in the intestine or in the blood, in which case they would simply fall off. These and many more hypotheses will have to be considered in the hope of finding the true explanation of the fact.

It ought not, I think, to be too hastily concluded that this abundant removal of epithelium is an indication of the occurrence of active *elimination* from the intestinal surface. For, in the first place, it must be remembered that the villi are not, in their normal state, organs of *secretion* or *elimination*, but active organs of *absorption;* while, on the other hand, Lieberkuhn's follicles, which open in the intervals between the bases of the villi, are secreting organs. Now these follicles, so far from being denuded, are choked with epithelium. It is possible, however, that there may have been a tendency upon the part of these cells to separate matter from the blood, but it seems improbable that the columnar cells which form the bulk of the cellular elements of the contents of the small intestine, and which unquestionably came from the villi, should have been engaged in such an office. Secondly, I would remark that the pabulum passes through the columnar cell in a direction *from* its free towards its attached surface, or *from the intestine towards the blood.* If, therefore, it eliminates or separates anything from the blood, the flow must take place through it in a direction the very opposite of that which is constant during its life. Is it not improbable that this should be the case? In short, it seems to me that evidence in favour of the view that the removal of the epithelium from the villi is an eliminative act is still wanting. By the denudation a raw surface becomes exposed, just as in the case of the cutaneous surface after a burn or scald, except that the villi are completely bared, which is not the case with the skin. No one would argue that the elevation of the superficial layers of the cuticle and the effusion of serum beneath consequent upon a burn resulted from elimination. We are to some extent acquainted with the several steps of the latter process, but we have not as yet learned much concerning the former.

The removal of the columnar epithelium from the villi, and the consequent destruction of the mechanism connected with absorption, are broad facts in cholera which deserve the most minute and careful study. We might well consider how this denudation may be prevented or retarded, and, having taken place, what fluids should be brought in contact with the naked surface—oily fluids, fluids containing salt syrup or glycerine, so as to make them of about the specific gravity of the serum, or ordinary serum itself, which last would probably be most efficacious—or whether it would not be better, until we know more of the matter, to let the denuded villi remain perfectly quiet, and allow the small intestine to rest, in the hope that the damage may be repaired. But is it not reasonable to hope that a thoroughly minute investigation into the circumstances which probably immediately precede this denudation of epithelium would enable us to form a notion of its nature, and to adopt means which were likely to restrain it? A knowledge of the changes occurring after the denudation and destruction of many villi will probably teach us much concerning the nature of "secondary fever," and enable us to place the patient under conditions most favourable to his recovery. The consideration of the mere fact of there being a raw denuded surface throughout a considerable extent of the alimentary canal suggests the propriety of not introducing anything into the intestines. The question concerning the alteration in the villi is a most interesting one, and it is worth while to spend time in searching for new facts, and in trying new experiments which may help us to answer it. But the subject is a very extensive one, and has many ramifications, each of which must be carefully considered in detail.

Remarkable changes have occurred in the smaller vessels, especially in the capillaries and small veins of the villi and submucous tissue, and these changes can be readily demonstrated. The blood-corpuscles appear to have in great measure been destroyed in the smaller vessels, and in their place are seen clots containing blood-colouring matter, minute granules, and small masses of germinal matter evidently undergoing active multiplication, but the nature of which has yet to be studied. Some of the arteries are contracted, but here and there small clots destitute of blood-corpuscles may be seen at intervals. Drawings of these will be published.

On the other hand, the nerves and the ganglia, so numerous between the muscular and mucous coats of the small intestine, exhibit a natural appearance, so that I should not be able to distinguish a ganglion taken from a cholera victim from one taken from a perfectly healthy person of the same age whose life was destroyed by accident. The nerves and ganglia, and the tissues for a short distance around the smaller vessels, are, in many situations, stained with altered and dissolved blood-colouring matter."—*Med. Times and Gaz.*, Aug. 4, 1866.

In a second article (*Med. Times and Gazette*, Aug. 18th), Dr. **BEALE** states that the columnar epithelial cells found in great number in the rice-water evacuations and small intestine after death from cholera, do not always exhibit the same characters, nor are they of the same size in every case.

He adds: "In almost all the cases of cholera I have yet examined there is evidence of chronic structural changes in the *tissues* of the intestines, and I think we shall be led to conclude that in most of the cholera victims important morbid alterations have been going on for months, and in some instances for years, before death. In some cases it is probable that, had the individual escaped cholera, he must have succumbed to some other malady within a short period of time. The columnar epithelial cells often exhibit evidence of chronic change; they seem to be stunted, and in many instances the nuclei are much smaller than in health. In the intervals between their attached extremities one fails to find those smaller and younger cells which in the healthy state gradually grow up to take the place of those cells which are removed and give origin to new cells, which in their turn become developed. So also it is to be observed that the masses of germinal matter so numerous near the surface of the healthy villus are almost absent in many of these cases of cholera. And there are other and very striking changes in the structure of the affected villi which I shall describe fully in other communications."

In one case, that of a child fifteen months old, Dr. B. found the epithelial sheaths of the villi very distinct and perfect, but the cells did not exhibit the characters seen in other cases. "They contained numerous oil-globules, some of which were of considerable size—a fact which perhaps justifies the inference that these particular cells were active and concerned in absorption shortly before death, which certainly is not usually the case in cholera. The above inference is confirmed by the characters of the villi existing in this particular instance. In most cases the fresh cells seem to be almost destitute of oil-globules, and many present a shrivelled appearance, as if they had not been very active for a long time before death. There seems, indeed, to be the same sort of difference between some of these cells in cholera and healthy columnar epithelial cells, that is observed between the epithelium of a cirrhose and that of a healthy liver or kidney." * * *

"Amongst the epithelial cells and upon their surfaces, if not in their substance, are multitudes of bacteria. Bacteria are found in the dejections during life, in the vomit, and in every part of the alimentary canal two hours after death.

"It is probable that these organisms are developed in the intestine in vast numbers during life." * * *

"The falling off of the epithelial cells cannot be attributed to the influence of bacteria, nor is it probable that these organisms are concerned in the production of cholera. Bacteria are found often enough in undigested or in imperfectly digested food. In dyspeptic infants they occur in vast numbers in every part of the alimentary canal; and in temporary stomach derangement the matters rejected by the stomach or passed per anum contain them in enormous quantity. They have been observed by Dr. Gibb even in milk immediately it was removed from the breast of the mother, and must have been developed while the milk yet remained in the mammary gland. They are always present in the fluids of the mouth, and are not uncommon in the contents of the stomach, though it is doubtful if they multiply in a perfectly healthy condition of the gastric mucous membrane. It is not very surprising that bacteria should be present in the cells of columnar epithelium in certain cases, seeing that their germs are always present in the old cells of squamous epithelium in the mouth.

They invade these columnar cells from without, and live at their expense, just as they invade the epithelial cells of the tongue, and as simple fungi invade the cells of higher plants and animals when these cells begin to decay or lose through disease their healthy power of resisting invasion.

"Bacteria are constantly found in every part of the living body where a tissue is no longer permeated by the fluids whose office it is to maintain it in a state of integrity. Soon after the currents of fluid have ceased, especially where the organic material is soft and easily decomposed, bacteria make their appearance, and grow and multiply rapidly. Nor is there a tissue or organ in the inmost parts of the body in which these organisms may not within the course of a few hours make their appearance. The germs are there; but so long as the normal state of things continues, these germs are prevented from being developed.

"These simple living organisms are without doubt destroyed in the healthy condition, although their germs may resist destruction. If the latter were to get into the healthy blood, they would not multiply, but towards the close of many exhausting diseases, both in animals and man, after the blood has undergone important changes, they are found in great numbers. It is, however, doubtful if in any case they can be regarded as the cause of the disease or the *materies morbi*, and it is far more probable that a change takes place before these organisms can multiply in the blood, and that a condition of things becomes established which is favourable to their growth and multiplication.

"There is then, I think, no good reason for supposing that the bacteria in such numbers in the alimentary canal in cholera have anything to do with this disease or with the falling off of epithelium from the intestinal or other mucous membranes. Bacteria are developed in organic matter which is not traversed and protected by the normal fluids of the body, and they invade the cells and textures in cholera after these cells and textures have undergone serious prior changes, just as they would invade textures removed from the body altogether. Nor would it be in accordance with known facts to infer that cholera was due to the invasion of some peculiar form or species of bacterium." * * *

"Cholera seems to be so constantly associated with the removal of columnar epithelium from the villi, that we have been led to look upon this as one of the *essential* phenomena of the disease. Although there may be no actual diarrhœa, this epithelium is found in quantity in the intestine after death. It may be said that this removal of epithelium occurs immediately after or only just before death, but the great number of columnar epithelial cells and entire sheaths of the villi so frequently found in the rice-water evacuation, giving to it its peculiar character, proves that such a notion is not tenable. Can cholera exist without the villi being denuded of their epithelium—is a question which, as far as I know, has not yet been answered, but which must be answered before we can form a correct notion of the nature of this most wonderful disease. I do not think there is any other morbid condition in which this striking change is observed—at any rate, to the extent or with the frequency it is met with in cholera. It seems, however, likely, that where those changes in the blood occur very quickly indeed, so as to cause death by sudden stagnation of the blood in the capillaries of important organs, there might not be time even for the removal of the epithelium from the villi, just as we may have death from smallpox or scarlatina without any eruption.

"With reference to the denudation of the villi, it must be borne in mind that the throwing off of epithelium is not confined to the villi of the intestine, or to that of the intestinal mucous membrane generally. The process affects the mucous membrane of the gall-bladder and larger gall-ducts; that of the bladder; ureters, and pelvis of the kidneys, as well as that of the Fallopian tubes, uterus, and vagina. In short, there seems a tendency to the removal of epithelium from the surface of all the soft, moist mucous membranes; not, it must be remembered, of the epithelium which is specially concerned in *elimination*, but rather of that which lines the *ducts* of glands and cavities which may be included in the category of the ductal portion of the different secreting glands.

"On the other hand, there is no evidence of the increased formation or more rapid removal of the secreting epithelium in the various glandular organs. The follicles of the mucous membrane of the stomach and intestine, those of the salivary glands and pancreas, the tubes of the liver, kidney, and other glands still retain their epithelium; nor have I been able to demonstrate in these varieties of glandular epithelium any appearances peculiar to cholera. Indeed, so far as I have yet been able to observe, it would be extremely difficult to distinguish many secreting cells taken from the bodies of cholera victims from perfectly healthy cells."

17. *Internal Temperature in Cholera.*—Dr. WEBER, of the German Hospital, Dalston, states (*Med. Times and Gaz.*, Sept. 1) that he has found in several cases of cholera the temperature of the rectum to be more or less above the average of health; that it had been in one case of severe collapse as high as 103°, while in the axilla at the same time it scarcely exceeded 95°; that with progressing recovery, the temperature in the rectum had decreased, while that in the axilla had increased.

18. *Aphasia with Right Hemiplegia, Softening in the Island of Reil, and External Frontal Convolution.*—Dr. SANDERS exhibited to the Edinburgh Medico-Chirurgical Society a drawing taken by Mr. Strethill Wright, of the base of the brain in a case of aphasia, recently fatal. The patient, Robina S., a female aged 50, had been under observation in the Royal Infirmary since 23d August last, and died on 17th April. She had been taken ill three days before admission. She was hemiplegic on the right side, and had entirely lost the power of speech, although her intelligence remained. She could only mumble inarticulately; no single word, not even "Yes," or "No," could be made out. Her writing consisted of unintelligible scratches. She was evidently anxious to express herself, and wept at her failure. Curiously, she could by signs make her husband understand her, and, by the nurse's report, they contrived to quarrel. There was no paralysis of the tongue or larynx; the hemiplegia affected chiefly the arm (which afterwards became rigid in the flexed position), also the leg and face on the right side, both of which, however, recovered a considerable amount of voluntary motion. She died of exhaustion, bedsores, and, ultimately, pneumonia. At the autopsy, the arteries at the base of the brain were found highly atheromatous. On the left hemisphere the principal lesion was softening, occupying the anterior and external portion of the island of Reil, and extending outwards and forwards into the under surface of the posterior part of the external frontal convolution (Broca's localization), which was affected, however, only to a small extent. The softening also penetrated inwards to the external and anterior part of the corpus striatum. There was also a small separate softening at the posterior part of the corpus striatum. There were also small detached softened portions in the white matter above and also behind the left ventricle. No lesion of the right hemisphere. Mr. Turner examined the brain along with Dr. S. This case partly supported, partly differed from, Broca's views. It suggested the island of Reil as a seat of the lesion in aphasia, the lobule of the island being of great importance in the conformation of the brain. Dr. S. mentioned, that since his case of aphasia had appeared in the *Edinburgh Medical Journal* of March, he had been asked to see three cases, and had received from friends information in regard to other three cases of aphasia; in all of the six cases, the aphasia was associated with hemiplegia, always of the right side of the body.—*Edinburgh Med. Journ.*, June, 1866.

19. *Atrophy and Degeneration of the Muscles of the Upper and Lower Extremities from Disease of the Spinal Cord.*—The following case of this was communicated to the Royal Med. and Chir. Soc. (June 12, 1866) by Mr. GEO. L. COOPER:—

J. J., aged 41 years, married, but his wife had no family; was much exposed to the weather in his daily occupation, at the same time had been a man of intemperate habits, and the subject of a long chronic cough. He was admitted under the care of the Author at the Bloomsbury Dispensary on February 14, and died on the 26th. He suffered from complete paralysis of the upper and lower extremities, with atrophy of the muscles of these parts. The symptoms were slow, but progressive. They commenced in the hands and feet, and extended to the arms and legs, and ended in total paralysis. His cough was severe, with purulent expectoration, to the time of his death, which took place on February 26. The post-mortem showed much distension of the coverings of the cord from fluid, with congestion of the pia mater in the cervical region, and considerable softening in the substance of the white columns. At the commencement of the lower third in the dorsal region the central gray substance contained a large dilated vessel on each side, surrounded by extravasated blood-globules; and the extremities of the posterior cornua were highly vascular, as also in certain parts of the gray substance there were patches of extravasated blood.—*Med. Times and Gaz.*, July 21, 1866.

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20. *Mode of Distinguishing between Nervous Idiopathic Albuminuria and the Albuminuria of Diseased Kidneys.*—The principle upon which M. Corlieu founds the test by which he distinguishes between these two forms is, that when the kidneys are healthy the urine possesses the smell of odorous substances introduced into the system. He says that if such substances as cubebs, turpentine, &c., be ingested, they will give their characteristic odour to the urine, in cases of albuminuria, provided the kidneys be healthy; but if the kidneys be diseased, as in nephritis, the odour of these substances cannot be detected, even though they have been previously introduced into the system.—*Lancet*, July 9, 1866.

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21. *Addison's Disease.*—Dr. Greenhow exhibited for Dr. Dickinson two supra-renal capsules infiltrated with cancer. They had been taken from the body of a man, aged 56, who had presented no discolouration of skin nor any of the constitutional symptoms of Addison's disease. Malignant disease of the spine was diagnosed during life, and on post-mortem examination cancer of the lumbar vertebræ, pancreas, liver, heart, and other organs was found, together with cancer of the supra-renal capsules. Dr. Greenhow likewise exhibited the supra-renal capsules, tongue, penis, and scrotum taken from the body of a man aged 55. Addison's disease had been diagnosed on the day of the patient's admission into hospital. The constitutional symptoms were characteristic, and besides slight general discolouration of skin there was deep discolouration of some cicatrices of burns, and also of penis and scrotum, and the tongue presented on its upper surface round the free edge irregular purplish stains. On microscopical examination the pigment causing these stains was found deposited in the papillæ in brown masses, the superficial layer of epithelium remaining uncoloured. The supra-renal capsules had undergone the peculiar morbid change usual in Addison's disease. In the lungs and other organs there were also tubercular deposits. Dr. Greenhow said that these two cases were good illustrations of the difference in the effects produced on the general health by different diseases of the supra-renal capsules. A discussion two years before at the Pathological Society, showing that doubts were still entertained as to the reality of Addison's discovery, and that these doubts were mainly fostered by the confusion of genuine and spurious cases, had induced him (Dr. Greenhow) to abstract from the British and Irish journals for the purpose of analysis all the published cases not of Addison's disease only, but of cancerous and other diseases of the capsules, and also of bronzed-skin without any disease of the supra-renal capsules. Subsequently his attention had been drawn to treatises on the subject by M. Virchow and M. L. Martineau, and he had translated from the original sources all the foreign cases quoted in their writings. In all he had collected 195 cases, which he had carefully analyzed, and had found that in 127 the supra-renal capsules had undergone the particular morbid change characteristic of Addison's disease. In the other 68 cases the capsules were either healthy or had undergone cancerous or some other morbid change, or the change in them was too obscurely described to enable him to determine its nature. With a few explainable exceptions, none of these 68 cases presented either the constitutional symptoms of the peculiar discolouration characteristic of Addison's disease. On the contrary, among the 127 genuine cases, 95 presented either well-marked constitutional symptoms or characteristic discolouration of skin, and 72 of these presented both. In 20 cases some of the constitutional symptoms or some dis-

colouration of skin existed, and in many instancss both, leaving only 12 out of the 127 cases which presented neither any characteristic symptoms nor any discolouration of skin. These 12 cases were all among those complicated with advanced tuberculosis or with other serious organic disease, which appeared to be the causes of death rather than the coexisting disease in the supra-renal capsules. On the other hand, only 5 of the 95 cases presenting characteristic symptoms or discolouration were found among the number complicated with serious non-tubercular diseases, and not one among those complicated with advanced phthisis. These facts seemed to be a sufficient refutation of the theory current abroad that the discolouration of skin peculiar to Addison's disease is due, not to the morbid change in the capsules, but to the general tuberculosis frequently associated with it. At the same time his (Dr. G.'s) analysis of the 127 genuine cases showed that while 46 cases were reported as entirely or virtually uncomplicated, and 16 as complicated with other serious diseases without tubercle, no less than 65, or an absolute majority of the whole number, were complicated with tubercular disease of lungs or other organs, in all gradations of development; so that it was impossible to avoid the conclusion of a very intimate relation between Addison's disease and the tubercular diathesis.

Dr. Quain said that he would wish to hear from Dr. Greenhow a clear definition of what he meant by the term "Addison's disease." The statistics quoted included a certain number of cases of bronzed skin without any disease of the supra-renal capsules, and a still larger number of cases in which there was more or less disease of the capsules and no bronzing of the skin at all. It did not seem altogether philosophical to take these two conditions and to say that it was their coexistence which alone constituted the disease. Dr. Quain then gave the particulars of an interesting case of fatal anæmia in a young gentleman, which he had seen with Dr. Addison, Dr. Bright, and Dr. Williams. It was one of the first two or three cases which had led Dr. Addison to state his views on the subject. In that case there was a deep ash-coloured pallor of the skin; the capsules were small, but not diseased. Dr. Quain thought there was still much room for investigating the character of the disease.

Dr. Greenhow said in reply that the cases collected by him were not of Addison's disease only, but of all diseases whatever of the supar-renal capsules which he had been able to find published from the time of Addison's discovery up to the end of 1865. He had only abstained from giving the well-known description of Addison's disease because he desired to be brief; but he thought it might be clearly defined as a particular lesion of the supra-renal capsules, manifested during life by a certain train of constitutional symptoms attended almost always by a characteristic discolouration of skin. No doubt his collection included a few cases of bronzed skin without any disease of capsules, but the bronzing was not the discolouration characteristic of Addison's disease. It included also a certain number of cases in which the capsules had been diseased without any discolouration of skin; but, with one or two exceptions, the disease either was not Addison's, or, being Addison's, it had been masked by some other predominant disease which appeared to have been the cause of death. As Mr. Hutchinson had suggested, when the discolouration was altogether absent, death had probably taken place at an early period of the disease. As regarded the relation between the symptoms and discolouration of Addison's disease on the one hand the characteristic change in the capsules on the other, he had no more doubt of its existence than he had of the existence of a relation between incompetency of the mitral valve or of a cavity in the lungs and the physical signs of these lesions during life. He had at present three cases of Addison's disease under observation, and he was bold enough to say that he should be quite willing to leave the question of the reality of Addison's discovery to be determined by the result of the post-mortem examination in any one of the three.—*Med. Times and Gaz.*, May 12, 1866.

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22. *Influence of Sex and Age in Determining the Liability to Asthma.*—Dr. HYDE SALTER has preserved notes of upwards of one hundred and fifty cases of asthma, which have come under his observation since the publication of his elaborate work on that disease, and tabulated them. He has been able from

these to throw light on many points in relation both to the clinical phenomena and treatment of that affection.

1. In regard to *sex*. Of 153 cases 51 were females and 102 were males, showing the latter to be more liable to the disease than the former, in the proportion of *two* to *one*.

2. In regard to the time of life at which asthma makes its appearance, Dr. S.'s cases show that:—

1st. That the time of life the most prolific of asthma is the time of measles, of whooping-cough, and of infantile bronchitis.

2d. That adolescence furnishes comparatively few cases, because the diseases of childhood, so apt to lay the foundation of it, are over, while the wear and tear and hardships of life, and the deterioration of the body produced by them and by time, have not commenced.

3d. That from this time exposure and hardship and time begin to tell, and show their influence by the increasing asthma rate reaching its maximum at middle life. But, it may be asked, why should the tendency for asthma to show itself increase up to forty, and then diminish? Why should it not go on increasing as life advances, especially as we know that the tendency of catarrhal and other agencies to produce inflammatory conditions of the respiratory mucous membrane does increase up to the very end of life?

This brings me to the fourth point, which is this: That this diminishing probability of asthma making its first appearance after middle life shows that it does not follow the same law as bronchitis, and that there is something necessary for its development besides vascular change in the bronchial tubes and other organic lung mischief. This other thing is doubtless the asthmatic tendency or idiosyncrasy; and the way in which the necessity of the asthmatic idiosyncrasy for the production of the disease accounts for the diminishing probability, as life advances, of its making its first appearance is this: As every year is added, an individual is decreasingly likely to be exposed *for the first time* to the exciting cause of the disease; if any one has the predisposing cause—the asthmatic tendency—within him, it is not likely he will travel far through life without the exciting cause presenting itself and bringing the disease into activity, and those only can reach advanced life without becoming asthmatic in whom either the asthmatic tendency is *nil* or feeble, or who have fortuitously escaped circumstances calculated to call it into activity. Such a number must, according to the doctrine of chances, be a constantly decreasing series. The diminishing number of cases is, in fact, an exact measure of the diminishing probability of a person with the asthmatic tendency postponing his first exposure to exciting causes to so late a date.

There is nothing in relation to asthma about which more misconception prevails than the time of life at which it is apt to occur. It is commonly thought to be a disease of old age, and we frequently hear the expression "as asthmatic as an old man." I believe there are two reasons for this error. One, that asthma, if it is not lost comparatively early in life, or if it comes on in middle life, is generally never lost, and therefore exists in old age; and thus many old people are truly asthmatic simply because they have never ceased to be so. The other reason is that chronic bronchitis—undoubtedly a disease of advanced life—is often mistaken for asthma: an old man coughs and wheezes and spits, and is said to have the asthma; but he has really chronic bronchitis; and although the bronchitis may have a little bronchial spasm superadded to it, or even a great deal, still it is essentially and substantively bronchitis.

3. *Duration.*—The longest time that asthma had existed in any of Dr. S.'s cases was sixty-four years. This period was reached in two instances; in one case the patient's age was sixty-nine, and he had become asthmatic at five; the other was seventy years old, and the asthma began at six.

There is no rule that the oldest cases have had the asthma the longest or the youngest the shortest time. Cases of the same age will have had their asthma very different times, while cases of equal standing will be found at various ages. Thus two of my cases aged seventy years had had their asthma one a year and one sixty-four years; while a third of the same age had had it just half his life, or thirty-five years. Again, two of my cases of equal standing, both having had asthma twelve years, were one aged sixty-nine, and the other fifteen—one an old man and the other a young girl—yet their asthmatic age was the same. In this way there is every variety—no rule. Twenty-seven of my cases were upwards of twenty years' standing, and of these, two had suffered from asthma thirty-four years, two thirty-five years, one thirty-seven, one thirty-nine, one forty-three, two forty-five, one fifty-three, and two sixty-four. Such numbers as these show how little tendency the disease has to shorten life.—*Lancet*, July 28, 1866.

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23. *Treatment of Delirium Tremens.*—Prof. GEO. JOHNSON, in a clinical lecture (*Lancet*, April 28th, 1866), expresses the following views as to the main points to be attended to in the treatment of delirium tremens, and the peculiar dangers to be guarded against:—

"First, bearing in mind the tendency to suicide, the patient must be constantly and carefully watched by a judicious attendant who has the power to exercise some moral control over him. The doors and windows should be secured, and razors, knives, and other means of mischief, put out of sight and reach. He should be kept quiet and undisturbed by noise or excess of light. It is better, if possible, to avoid direct physical restraint. If the patient be violent and difficult to control, secure the services of two attendants rather than have recourse to a strait-waistcoat. I have known more than one instance of sudden death from exhaustion induced by violent struggling against the restraint of the strait-waistcoat, and I have also known more than one instance of fatal exhaustion from struggling with injudicious attendants. Not long since a medical friend called one morning and desired me to go with him immediately to see a publican whom he had just before seen with an attack of delirium tremens. On entering the room we found the patient gasping for breath and evidently dying; and we learnt that immediately after my friend had left his patient, less than half an hour before, he got out of bed and wished to leave the room. Three strong men who were present resisted his attempt to go out; a struggle ensued, and in the midst of it he suddenly fell exhausted as we saw him, and he quickly ceased to breathe. You see, then, how great is the risk resulting from violent exertion on the part of these patients, and how important it is to avoid any restraint which is likely to provoke a struggle. In some instances a patient who cannot otherwise be kept in bed will remain perfectly quiet under the restraint of the waistcoat; but while he is thus restrained he must on no account be left alone. He may begin to struggle at any moment, and this struggle may cause rapid and fatal exhaustion.

"To sum up, then, what I have said on the subject of restraint, the guiding principle is to prevent any violent exertion on the part of the patient—any struggle either with the strait-waistcoat or with the attendants. A padded room, when it can be had, is the safest and most effectual means of restraint for a violent patient.

"Having insured the safe custody of the patient, we have next to consider the method of treatment. Now, in the first place, bear in mind that delirium tremens is one of those diseases from which recovery may take place without any assistance from drugs. Medicines are of great use in the treatment; but they are not essential. It is, however, of essential importance that the delirious patient should be *fed*. The history of the disease teaches us that the immediate cause of the delirium is the exhaustion consequent on a defect of wholesome nutriment; and, until this defect be supplied, all attempts to procure the wished-for sleep, which is so generally followed by a cessation of the excitement, are often useless and worse than useless.

"Whatever else you do or leave undone, never neglect to feed a delirious patient. If he will take food when he first comes under treatment, give it immediately, and let it be repeated as soon as he will take it. If there be, as there often is, a disinclination for food, with nausea and a coated tongue, an emetic of ipecacuanha, followed by a dose of calomel and colocynth, or a saline laxative, will be of use as a preparation for food and for the opiate, which may best be given at bedtime. You may then give from a half to one drachm of tincture of opium, which I believe to be a better soporific in these cases than the salts of morphia; and a smaller dose may be given in three or four hours if necessary. The first object is to procure sleep. But here I wish to impress upon you the necessity of great care and watchfulness. Do not make rash attempts to force on sleep by repeated large doses of opium. These attempts will often fail to procure sleep, and they may kill the patient. It is an undoubted fact, that opium in many of these cases has no soporific effect whatsoever. And not only in cases of delirium tremens is opium uncertain in its operation. When we give opium in ordinary cases of disease as an anodyne or a soporific, we can never be sure that it will cause sleep. In a considerable proportion of cases opium *prevents* sleep, and makes the patient more wakeful than he would have been without it. Another not uncommon effect of an opiate is to cause nausea and faintness. These two effects of opium should always be borne in mind in the treatment of delirium tremens. Remember that when opium fails to act as soporific in delirium tremens it is not inert, and must not be given in repeated large doses as if it were; while it fails to procure sleep, it may be exerting a powerful depressing and paralyzing influence upon the heart. The symptoms of opium thus acting on the heart are these: The patient continues wakeful, excited, and delirious, but grows rapidly weaker; the pulse becomes quick, small, and feeble; the pupils are contracted; the skin is bathed in a profuse sweat; and, if the opium be continued in large and frequent doses, the patient rapidly sinks, but remains wakeful and conscious until perhaps within a few minutes of the end. The opium in these cases acts as a powerful sedative on the heart, and in proportion as it does this it fails to exercise any soporific influence. If you find that opium is acting thus injuriously, you must immediately discontinue it, and give liberal doses of brandy, or the stimulant to which the patient has been accustomed. Full doses of quinia, too, will be of use as a tonic; and nutriment, either in the liquid or solid form, should be freely given.

"It is an undoubted fact that the soporific effect of opium is often much more certain and decided when the medicine is given with some alcoholic stimulant than when it is given alone; and I have seen not a few cases of delirium tremens in which, after repeated large doses of opium have failed to procure sleep, a liberal allowance of the patient's accustomed stimulant, more especially when combined with food, has been followed by a long sleep, and this by an entire freedom from delusions and delirium. I scarcely need impress upon you that these unquestionable facts are quite irreconcilable with the hypothesis that the proximate and essential cause of delirium tremens is the presence of alcohol in the blood. If the patient refuse to take the opiate or other medicine which you are anxious to give, he may sometimes be induced to take it mixed with his beer or other beverage. Both food and stimulants may be given by the rectum in case of need; and morphia may be introduced subdermically; but this mode of giving morphia requires at least as much caution as the administration of opiates by the mouth.

"Chloroform vapour has the immediate effect of quieting the delirium and excitement; but the quiescence is only temporary, and I have seen no permanent good result from it. The fat and flabby state of the heart in these cases is a bar to the incautious administration of chloroform.

"When there is much flushing of the face and heat of the scalp, cold lotions or iced water in a bladder may be applied to the head with advantage.

"You may have heard or read of large doses of tincture of digitalis having been given for the cure of delirium tremens. I consider this a very dangerous remedy. I have no doubt that some patients have recovered in spite of the treatment. When half-ounce doses of the tincture have been given, it is to be hoped that the digitalis was of bad quality; the patient would then, at any rate, have the benefit to be derived from the stimulant action of the rectified spirit. I have heard of several instances of sudden death after the administration of large doses of digitalis, and it is only natural that these calamitous results should not, as a rule, obtain equal publicity with the cases of apparently successful treatment. A consideration of the natural history of delirium tremens suffices, I think, to show that the treatment by large doses of digitalis is irrational and dangerous.

"To sum up, then, the main practical points which I have endeavoured to impress upon you. Bear in mind that an attack of delirium tremens, as a rule, subsides in a few days without help from medicines of any kind. Let the patient be carefully watched and guarded and fed, and the excitement and delirium will gradually subside. In many instances opium is a valuable aid in the treatment, and it will often cut short the disease; but remember that it is powerful for evil as well as for good. Its use, therefore, requires much care and discrimination. Some form of alcoholic stimulant will often be of great use in calming the nervous excitement and procuring sleep."

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24. *Treatment of Pneumonia.*—Dr. ALEX. SMITH has published (*Edinburgh Medical Journal*, July, 1866) some interesting facts in regard to the treatment of pneumonia, deduced from his experience in 108 cases in soldiers, treated at various stations in Canada. There were 3 deaths, giving a mortality of 1 in 36.

In regard to bloodletting, he says: "My experience of the effects of bloodletting convinced me that its employment at the outset of pneumonia in its sthenic form was attended with most beneficial results, not only in shortening the duration of the disease, and rendering convalescence satisfactory, but also in giving an amount of certainty and uniformity to the results of treatment which could not be attained by the employment of any other combination of remedies. As to its power in "cutting short" the disease—if by this term is meant to be expressed the probability of its at once arresting, and as it were stamping it out—my own experience would go to show that its employment is not attended with any such result. In proof of this I may mention that so soon as I became aware of the import of the condition of the respiration, which is first observed at the outset of pneumonia, I attempted, by early bleeding before the disease had advanced beyond the stage indicated by obscurity of the respiratory sounds, to arrest it in that of engorgement. In no case, however, was this practice attended with the result desired; but, on the contrary, in every attack so treated, instead of being altogether prevented, small crepitation seemed to undergo an earlier development—an occurrence which may perhaps be held as in some measure bearing out the accuracy of the views I have expressed as to the nature of the early stage of the disease in sthenic attacks. The subsequent progress of all such cases bled was otherwise invariably satisfactory.

"I would still, however, feel inclined to consider this question in the light of an open one, and to believe, until distinct proof to the contrary shall have been produced, that bloodletting practised soon after the occurrence of the rigor may possibly at once arrest the disease. I am the more inclined to this view of the matter, because Dr. Jameson, my colleague in the 47th Regiment, informs me, that in one case which he bled freely immediately on the man's admission into hospital, and within a very short time of the occurrence of an attack of rigor which, from all the attending circumstances, and happening as it did at a time when pneumonia was prevalent, among the men of the corps, appeared to be the initial symptom of an attack of that disease, no further indisposition followed. This may or may not have been a case which, if it had not been so treated, would have proved one of pneumonia; but still I believe the fact is worth recording.

"A perusal of the records of cases which I have given above will show I think, upon the whole, with considerable clearness, that it was by limiting the stage to which the diseased action advanced, rather than by affecting the extent of lung to be attacked, that bloodletting manifested its power to shorten the duration of the disease. That it also influenced the amount of lung attacked, however, appears evident, from what was found to have happened in some of the fatal cases, neither of which were bled at the outset of the disease. It may be here stated, with regard to the extent of lung affected in cases early bled, that it amounted, as a general rule, to from one-half to three-fourths; and that in respect of the part first attacked, in no instance did the disease begin at the apex.

"After having most carefully watched the whole course of the disease in attacks where bloodletting was employed at the outset, I feel satisfied that in no case so treated did red hepatization take place; both the exaggerated respiratory sound heard near the acme of engorgement, as well as the absence of evidence of the entrance of air, except during forced respiration, which frequently for a few hours preceded the setting in of small crepitation, having been, as already so often stated, unconnected with any degree of actual consolidation. Neither were the bronchitic sonorous râles occasionally audible along with large and small crepitation near the middle of the lung, in the course of some of the cases, confounded with the blowing sound of bronchial respiration heard when true hepatization was present. The facts of greatest importance, however, noticed with reference to the employment of bloodletting, were the rapidity with which such cases recovered in proportion to the severity of the attacks, and the uniformity of the results observed on a review of the whole cases so treated, as compared with that obtained in the milder and more asthenic attacks in which bloodletting was not made use of. This has been shown by the tabular statements given at an earlier part of the paper.

"A further consideration, possessing also considerable practical importance, is the fact, that in cases not bled it was found that there existed, throughout the greater part of the attack, a danger that a fresh accession of fever, and a rapid advance to hepatization, might not only suddenly occur, but do so at a period of the disease when good results from bloodletting, if it should then be employed, were but little likely to be obtained. In conclusion, I would, however, beg that it may be distinctly understood, that whilst advocating the employment of bloodletting at the outset of sthenic cases of pneumonia, such as are seen in young and previously healthy soldiers, and whilst maintaining also from actual observation that the good results which follow such a mode of treatment surpass in a marked degree those obtained from any other combination of remedies, I do not in any way call in question the value of that mode of treatment termed 'restorative,' as applied to a particular class of cases, and which has been employed with so much success in the management of the pneumonia as seen in civil hospitals in Britain.

"It would appear, however, from such limited details as have been given of the cases, on the results obtained, in which this plan of treatment has been based, not only that the attacks were of an asthenic character, but it may also be inferred that, in a large proportion of instances, the disease had advanced to the stage of actual consolidation before it was brought under medical treatment at all. On this supposition, therefore, these were cases in which bloodletting would in all probability have been inadmissible, but they were exactly such as would derive benefit from the description of treatment in question. My own experience of pneumonia would, accordingly, lead me to conclude that it was only in such asthenic cases as those above referred to, modified as they must be by the minor degree of intensity of their exciting cause, as it prevails in a climate equable on the whole as that of Britain is, and influenced also as they cannot fail to be by conditions of food, clothing, locality, and occupation, that this plan of treatment can be advantageously employed.

"In sthenic cases, such as came under my own observation in Canada, and of which it is possible that examples may occasionally be met with at home, facts have convinced me that a restorative plan of treatment could not be exclusively employed without the risk of at least a considerable mortality, or at all events the almost certainty of a recovery protracted beyond what it would have been had bloodletting been made use of. I would further add, that what I have learned in the course of this inquiry induces me to believe that much of the confusion and diversity of opinion which have of late years arisen on the subject of the pathology and treatment of pneumonia has been the result of a somewhat restricted view of the extent of the field of inquiry embraced by the subject under investigation, and the too resolute belief, not only that the asthenic pneumonia, which has of late years supplied the large proportion of the cases met with in this country, is the sole form in which inflammation of the lungs prevails now, but is even the only type in which that disease has existed at any previous period. I must at the same time beg leave to be pardoned if I venture also to hint my suspicion that some portion of this state of opinion may likewise be due to the condition of the lungs which exists during the highest state of engorgement having been confounded with that state of actual hepatization the occurrence of which renders it absolutely essential for the cure of the disease that the next highest stage—that of suppuration or gray hepatization—should also follow."

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25. *Treatment of Diphtheria.*—Dr. W. LINDESAY RICHARDSON, of the Ballarat Hospital, Melbourne, has in the course of seven years had under his care two hundred and twenty cases of diphtheria. Of these, one hundred and seven occurred in children under ten years of age. The number of deaths was eighteen; two only of which were in-patients about five years old. This mortality (8.2 per cent.) is, Dr. Richardson observes, very little in excess of that of measles (7 per cent.). As to the treatment, he observes that there is no disease more amenable to treatment in patients over five years of age, nor any in which a favourable termination may more speedily be predicted. The treatment consisted in 1859 of the free use of caustics from the first, chlorine gargles and iodide of potassium. This was not successful. In 1860 and 1861, the chlorate of potash and hydrochloric acid mixture was used, still occasionally applying caustic. Since then, local applications have been completely disused, and the muriated tincture of iron has been given in full doses, and latterly the chlorate of potash in powder with sugar dry on the tongue. Dr. Richardson used the nitrate of silver, in the first years of the appearance of the disease; then hydrochloric acid and honey; but his experience is that all applications to the throat are useless, and even hurtful. The mixture containing free chlorine he considers certainly of great value, but its strength appears to vary and its action is not certain. The muriated tincture of iron was first brought into notice, as a remedy in this disease, by the author's friend, Mr. T. H. Smith, of St. Mary's Cray, in a paper that appeared in the *British Medical Journal* some years ago. Dr. Richardson has found it most reliable in all cases of membranous diphtheria. The fever symptoms are no indications against its use, as under it the pulse falls. In a case under treatment, one day the pulse was 146; the patient was put on twenty minims every two hours, and next day it was 96, with an improving throat. It did not, however, appear to exert any speedy influence over the disease when it had extended into the trachea. It was necessary then to resort to emetics, to expel the false membrane as it forms. Dr. Richardson prefers common salt and sulphate of copper; succeeding these, the inhalation of turpentine on hot flannels, with the constant application of compresses around the throat, are most beneficial. Dr. Richardson performed tracheotomy in one case, and assisted at it in several, but always without success. In putrid sloughing throat, he finds that neither caustics, nor tincture of iron, nor solution of chlorine, nor quinia, exert any influence; and after trying various remedies he at last hit upon what he believed a most valuable remedy, chlorate of potash in powder. It appears to be unactive, even in a saturated solution; but in large enough doses, mixed with sugar, dry on the tongue, it exerts an immediate influence. In mixed or relapsing cases, an alternation of the two meets the case. On the important question of support to the patient, there can be no difference of opinion. Dr. Richardson has not examined the urine in every case; in those in which he did, he did not find any trace of albumen, nor any deposit more abnormal than lithates and purpurates. Six persons suffered at different times from two distinct attacks. Recovery as a rule is rapid, and paralysis as a sequela has been rare; three cases only having occurred.—*Australian Med. Journ.*, July, 1865.

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26. *The Milk Cure.*—The *Edinburgh Medical Journal* for August of the present year contains an article on this subject by Dr. PHILIP KARELL, Physician to the Emperor of Russia, translated by Dr. Carrick. After giving a brief historical sketch of the use of milk as a remedial measure, he says: "With regard to my own practice, I have, after fruitlessly trying all sorts of remedies in many chronic and obstinate diseases, at last succeeded in thoroughly bringing the alimentary canal, that seat of so many diseases, under my control. I did this by administering milk according to a new method. The results which I have thus obtained tempt me to publish my observations with reference to the efficiency of this mode of cure, provided, of course, that it be administered with

method, and by a person of experience. And in the first place, then, must we attribute the beneficial influence of milk in certain serious illnesses merely to its nutritive qualities, or to some occult medicinal virtue? I cannot pronounce in favour of the one or of the other hypothesis. It must be remembered, however, that milk and chyle resemble each other very closely. After a great deal of experience, I have arrived at the conclusion that in *all dropsies*, in *asthma*, when the result of emphysema and pulmonary catarrh; in obstinate *neuralgia*, when its cause lies in the intestinal canal; in diseases of *the liver* (simple hypertrophy and fatty degeneration), and generally in diseases where there is faulty nutrition, often a consequence of obscure subacute inflammation of the stomach or intestines, followed by affection of the nervous centres—in all these cases I consider milk as the best and surest of remedies. Even in those cases where the dropsy is the result of organic heart disease, or of old-standing liver complaint, or of far-advanced Bright's disease, I have seen very marked improvement take place, which also lasted a considerable time. But if, unfortunately, we are unable to cure organic disease, shall we not have conferred a great benefit on poor anasarcous patients if we reduce, with a promptitude little hoped for from other remedies, the distressing symptoms of œdema?

"If, in giving a general definition of the milk cure, we call it a *nutritive cure*, it by no means follows that it should only be administered in diseases dependent upon a perverse nutrition. It might as well be defined as a *sedative* cure, for it is very often useful in those cases where Valsalva would in all probability have employed fasting and phlebotomy. A more exact definition, perhaps, would be, that milk, when methodically administered, is a *regulator of nutrition*. It might perhaps be urged that milk is a well-known remedy, and that every physician uses it in appropriate cases. I admit that all medical men are sufficiently well acquainted with milk as a nutritive agent, and as an antidote, but I speak from experience when I assert that in general the cure by milk, *scrupulously administered, and in strictly measured doses*, is not sufficiently, and only very rarely, recognized as a sovereign and useful remedy.

"I have frequently, during the last fifteen years, been called into consultation in cases which were thought hopeless, and in many of which I recommended the milk cure, which had never been resorted to during the whole course of the malady. I had prescribed, even before that time, the employment of milk, but without regulating its administration. It was only by degrees that I arrived at a methodical system of treatment."

The following is Dr. K.'s mode of treatment:—

"I generally commence the cure by employing milk *alone* and forbidding all *other kind of nourishment*. I proceed with great caution in prescribing for the patient, three or four times daily, and at *regularly observed intervals*, half a tumbler or a tumbler, *i. e.*, from two to six ounces of skimmed milk. Its temperature must be made to suit the patient's taste. In winter they generally like tepid milk, heated by placing the tumbler or cup in a vessel filled with hot water. In summer they generally prefer it of the same temperature as the surrounding atmosphere. They should not gulp it all at once, but take it slowly and in small quantities, so that the saliva may get well mixed with it. Of course, the milk must be of good quality. That of town-fed cows has an acid reaction; that of country-fed cows is better, because its reaction is generally neutral. If the patient digest the milk well, which is proved by the feces becoming solid, I gradually increase the dose. The first week is the most difficult to get over, unless the patient has a strong will and firm faith in the cure. During the second week, two ordinary quarts are generally administered each day. If the cure take its regular course, then the milk must be drunk four times daily—at eight in the morning, at noon, at four P. M., and at eight P. M. If the patient desire it, I change the hours, but I always insist on regular intervals being observed, for the patient will think lightly of the cure if he be not ordered to observe some regularity while subjected to it. No confidence can be inspired, and no cure expected, if the physician says to his patient, 'Drink milk in whatever quantities and whenever you wish.'

"When obedient to the physician's orders, the patients complain neither of hunger nor thirst, although the first doses appear very small to them. If, instead of four cups of skimmed milk, a person afflicted with a severe illness takes four large tumblerfuls of unskimmed milk, you may be sure he will not digest it, and his confidence in the remedy will be shaken at the very commencement. * * *

"But it must not be supposed that such an effect can generally be produced when nothing is administered except small doses of milk. I have placed patients who were taking milk in minute quantities also on beef-tea, white bread, and water; but I never observed the same satisfactory results after this mode of treatment. The cure never was complete when allowed anything except milk to be taken for dinner. Sometimes, when the invalid had arrived at taking from ten to twelve glasses per day, I observed a return of his illness. I had then to commence the cure anew, by prescribing milk in small doses. At the beginning of the treatment, the patient's bowels are frequently constipated, which I consider of good augury. The feces become very hard, in consequence of the absorption of the fluid particles of the milk. This may be remedied by warm water injections, or by the use of castor oil or rhubarb. Persons suffering from flatulence are soon relieved of it by the milk cure. If the constipation be obstinate, I order the addition every morning of a little coffee to the dose of milk, or, towards four o'clock P.M., stewed prunes or a roasted apple. If, on the other hand, diarrhœa and borborygmi be the result of this mode of cure, it proves either that the milk was too rich, or that it has been administered in too large doses. If the diarrhœa does not arise from ulceration of the intestines, it is sure to be cured by strict observance of method in this treatment.

"Feverishness is no contra-indication to its use. If the patient feel very thirsty, I allow him to drink water or Seltzer-water. If he have a strong desire for solid food, I allow him, at the end of the second or third week, a little stale white bread with salt, or a small piece of salt herring. At four o'clock, *i.e.*, his dinner hour, the patient may, as in the morning, take a small quantity of stale bread. Once a day, instead of pure milk, I give him some soup made of milk and oatmeal. After continuing this treatment for five or six weeks, it may be modified (according to circumstances) by our allowing only milk thrice daily, and once a week a steak or chop. I have found that raw meat is easiest to digest.

"The strongest opposition to treatment I have generally experienced from the patients themselves, and the cause is easily explained. If a person, suffering from some chronic ailment, has already been subjected to various modes of treatment, without having been cured by any one of them, and if the milk cure be suggested to him, which, in his opinion, can lead to no improvement, he thinks it is the same as a verdict which declares: 'You are lost, and medicine cannot save you!' I have sometimes seen nervous patients grow seriously alarmed, and request time to reflect whether they should subject themselves to the treatment or not. Thus the patients either assert that milk is repulsive to them, or that they are unable to digest it; this one because he has always been troubled with his liver; another because he smokes; while a third is afraid he will die of hunger, or pretends that he has already tried the milk cure, but was unable to continue with it because of the disagreeable effects it produced. Others ask what purpose the milk cure can serve, when other medicines have done little, if any good. My answer then is that milk is a food easy of digestion with every person, provided it be given with precaution, that it be of good quality, and administered in definite doses; that it is the first food of man, and that a new-born infant shows no dislike to new milk. To die of hunger, even when taking nothing but milk, is impossible, since there are people who take no other nourishment. In milk are united all the elements necessary for the nutrition of our body, and besides this substance is easily assimilated. Lastly, I add that long experience has convinced me that milk is an energetic remedy in many diseases, and that in some cases I prefer it to any other remedy. Thus I am rarely unable to persuade the patient to follow out my advice; and in the majority of cases, notably those of dropsy, I have generally had the satisfaction of receiving, in a very short while, the sincere thanks of the patient for the speedy relief he felt. * * *

"In summing up the phenomena always observed among the patients cured or treated by other physicians and myself, I must enumerate: An intractable state of the blood, impoverished to the utmost extent, and general dropsy; disordered innervation, assuming the forms of hysteria, or hypochondriasis; obstinate dyspepsia, neither the result of congestion of the stomach nor of cancer of that organ; in fact, catarrhal, rheumatic, and gouty affections, as also nervous maladies not the result of a *local disease*, but of quantitative and qualitative defects in the fluids; or, to speak, more clearly, a constitutional disease. If the cause of the disease was apparently situated in the organs of digestion, the more strongly was I tempted to try this cure. I have thus cured, or very much relieved, chronic irritations of the pharynx and of the œsophagus, ulcers of the stomach, and similar diseases of the digestive tract. These *gastric cases* formed the greater portion of the 200. Among these, satisfactory results were obtained in a very short time. The desponding patient became lively, the gloomy countenance brightened up, the big belly decreased in size, and, as a consequence, many other unpleasant symptoms disappeared; in a word, the patient felt quite a new man.

"And even where the seat of the malady was not always as clear as in the cases above cited; but where the disease of any organ seemed to be connected with some derangement of the digestive tract, I have invariably tried the milk cure. For I thus produce a good result simply by regulating the diet, and by excluding indigestible articles of food. And I have thus frequently had the satisfaction to see a complete cure effected by such simple means in cases where deep-seated organic disease was suspected. My own experience, and that of other physicians, has shown that great improvements, and even almost a complete feeling of health, have attended this treatment, when employed in cases of organic disease of the heart, of advanced degeneration of the kidneys, etc. Taking into consideration the fact that hypertrophy of the heart and the central congestion, as well as increased bronchial secretion which result therefrom, are frequently caused by disorder of the abdominal circulation, I think I have found an exact indication for the milk. I have modified the milk cure according to circumstances in treating plethoric persons.

"The fatty degeneration of the arteries, and the consequent friability being so frequently one of the determining causes of apoplexy, I think we shall find an exact indication in that disease for the use of milk. Neither can I say that constitutional debility was common to all patients whom I placed under the milk cure. On the contrary, I have made persons of a florid complexion undergo the treatment—persons of muscular build and a full pulse, who are generally ordered a temperate regimen, and who, to prevent congestion and apoplexy, take bitter and saline solutions with benefit. For advanced tuberculosis we have no remedy. In cases where this disease is complicated with tubercular ulceration of the intestines, I cannot foretell very good results from the use of milk.

"Fever is no contra-indication to its use. The utmost caution, however, should be used when milk is administered in such cases. At the commencement the doses should not be increased too speedily, for the patient's stomach will not absorb more milk than it can digest.

"To sum up, I have already strongly expressed myself against the practice of extolling the milk cure as a panacea; nevertheless, I feel no hesitation in declaring that the number of cases for which I prescribed the milk cure with a great degree of confidence is very considerable, and that in these cases I could have expected no good results had I resorted to any other mode of cure."

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27. *Citrate of Soda in Diabetes.*—M. Guyot-Dannecy recommends citrate of soda, in daily doses of half a drachm to a drachm, as a remedy in diabetes. It has been shown by analysis that sugar disappears from the urine when this salt is used with the food instead of common salt. It is also known, since the researches of Wœhler, that the alkaline salts of organic acids, when given in doses too small to produce purgative effects, are absorbed, and, their acid being burned up in the respiratory process, are eliminated by the urine as carbonates. Hence citrate of soda may, without interfering with the gastric acid in the same way as alkaline carbonates, place the system under the influence of an alkaline carbonate, which is indispensable to the interstitial combustion of the glucose of the food.

The efficacy of this remedy, and its superiority to the prolonged administration of bicarbonate of soda, have to be proved by clinical experience.—*Brit. Med. Journ.*, May 19, 1866, from *Bull-Gén. de Thérap.*, April 15, 1866.

SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

28. *Osteo-Myelitis.*—This affection, which is one of the frequent causes of death after amputation or other injuries or surgical operations involving section of bone, attracted the attention of a number of our surgeons during our late war, and was made the subject of a highly interesting paper by Dr. H. Allen, published in the No. of this Journal for January, 1865.

Dr. J. FAYRER, in his address before the Bengal Branch of the British Medical Association, published in the *Indian Annals of Medical Science*, October, 1865, states that with the view of illustrating this affection, he has noted 32 amputations performed within a period of two years in his practice in the wards of the Medical College Hospital. These amputations "were all capital operations, *i. e.*, of the upper and lower extremities, either at the joints, or through the continuity of the long bones. They were—*one of the hip, three of the thigh, ten of the leg, four of the ankle* (*Syme's*), *five at the shoulder-joint, five of the arm, four of the forearm. Of these, thirty-two in all, three* were secondary amputations, and of the *number, fourteen lived, fifteen died. Of the deaths, nine resulted* from pyæmia, the consequence of osteo myelitis, *three from pyæmia* not depending on bone disease. There were *six deaths* from other causes, such as tetanus, gangrene, exhaustion. Now it will be at once recognized that this proportion of deaths from pyæmia, depending on bone disease, is something unusual—something very different to the ordinary death returns of other hospitals. It is this, therefore, that I wish specially to call your attention to, not only in regard to its pathology, but with respect to the treatment, which involves a question of amputation of much importance.

"The subject of acute suppuration in bone is one which has, apparently, not attracted much attention hitherto in this country, nor, indeed, has it been, so far as I am aware, so much studied anywhere as by the French surgeons, who have given it the name of *osteo-myelite*, by which we also now distinguish it. M. Jules Roux, Surgeon in Chief of the Great Naval Hospital of St. Maundrièr, in Toulon, is the authority to whom we are indebted for the most elaborate account of this important subject in both its pathological and surgical bearings. His experience was chiefly gained in the treatment of the wounded of the French-Italian war, who were sent to Toulon for treatment—all, consequently, cases of disease or injury of some standing; and the results of his observation and treatment are interesting in the highest degree: for they not only establish the recognition of the disease as a formidable result of operations on bone and of injuries, such as those inflicted by gunshot wounds in bone, but they point to the necessity of thoroughly re-considering the question of amputation, and fully, in my opinion, tend to confirm the view that the site of the amputation has, not less than the time at which it is performed, much to say to the mortality.

"M. Legouest tells us that at the Hospital of Dolma Bachè in Constantinople, out of 639 cases of amputation, M. Salleron lost 224, or little less than one-fourth. The amputations in the continuity of the bone were 490; resulting in 192 deaths from purulent infection of 1 in 21.2. The disarticulations, in number 149, resulted in 32 deaths, or 1 in 42.3.

"M. J. Roux records, in his practice in St. Maundrièr, the following remarkable success in the treatment of serious cases of gunshot injuries requiring amputation, all being secondary amputations.